ELECTRONIC NEWSLETTER ON
REASONING ABOUT ACTIONS AND CHANGE

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Today

The present Newsletter reports an interesting refereeing situation where one of the Anonymous Referees has a number of objections to one of the submitted reference articles. In line with the ETAI's policy his report is made openly and subject to answers and continued debate. The details follow.

ETAI Publications

Discussion about received articles

Introduction to the Fluent Calculus

by **Michael Thielscher**

Comment from: **The Editor**

The present introductory remarks refer to the statements by Anonymous Referee number 1, which are listed below.

The standard procedure in the ETAI is that we have the discussion during the open review period, and referees only make recommendations for a yes or no decision on acceptance. However, we also wish to accomodate the case where a referee adds substantially to the discussion about an article, as is the case here. In keeping with the ETAI's idea, we wish to make these comments open to the author (to give him a chance to answer) as well as for the readers of the Newsletter and web page who may also contribute to the discussion.

Both the author's contribution and the referee's comments must of course be interpreted in the context for which the article was submitted, that is, as a reference article. The explanation of the reference article concept, and the refereeing criteria for acceptance, can be found from the general ETAIJ web page by clicking "For Authors" and "Referee". The first criterium was specified as follows:

- Does the article represent a tradition or "approach" where there is already a sufficient volume of work in the field?

Our assumption was, of course, that any approach that satisfies this requirement should already have established its "case" which everyone does not necessarily agree with, but in case of controversy the approach should have its defense and its defendants. Refereeing with respect to the pros and cons of the approach would not then be required. However the first part of the comments by Anonymous Referee 1 do question the approach as such.

Since reference articles were not supposed to include a defense of the respective approach, it is appropriate to open the discussion at this point and to obtain the author's point of view. Other contributions to the discussion are also welcome.

The second part of the Referee's comments refer to refereeing criteria number 2 and 5:

- Does the article concisely specify the assumptions, motivations, and notations used in that approach? Does it correctly capture the assumptions, etc. that have been used and are being used?
- Is the article pedagogical and sufficiently easy to read, but at the same time precise and correct?

The remaining criteria include the question whether the article can effectively be used as a standard reference for the notation and the basic concepts of the approach being presented, for use in other articles using that approach.

Comment from: **Anonymous Referee 1**

Objections to the approach

The idea in this approach is a fairly simple one. 'States' (it would be better to say 'state descriptions') are lists of atomic assertions encoded as terms, and one uses a unification trick to ensure that any things in the list that aren't explicitly mentioned in the 'update axiom' get automatically transferred to the list which describes the post-action state. However, the 'deletion' here depends on an assumption about unification which isn't logically valid: that if two terms don't unify, they have disjoint referents.

Apart from the logical difficulties, this is computationally trivial (sub-STRIPS) and unoriginal. It is indequate for realistically complex domains. For example, take the toy blocks world as given, and add 'Above' to the vocabulary, defined recursively in terms of 'On'. The update axiom now needs recursion. Examples like this have been around for a quarter of a century. In general, the FP is only nontrivial when one has a vocabulary such that some aspects of the state description depend on others in ways that vary with the particular situation: in STRIPS terms, not all the vocabulary is part of the core. If not, then one can just have explicit add and delete lists for each action description, which is essentially what Thielscher is doing here, but in Prolog.

It doesn't have a clear semantics. The account given uses logical terminology, but it doesn't really use first-order logic. The extended unique names assumption is so strong that it renders equality trivial, as the authors take pains to show.

The limitations of the techniques being used ought to be carefully stated. In particular, the authors should refer to the STRIPS planner developed at SRI in the 1970s and explain how these techniques are inferior to those used by it.

Comment from: **Anonymous Referee 1**

Objections to the presentation as a reference article.

I confess it was very hard to finish the first page without getting very puzzled. The text assumes the reader shares nonstandard definitions of technical terms from obscure conference papers, and the illustrative example is very hard to understand.

Example 1 uses a (binary!) function symbol $\circ$ which applies to (three!) arguments referred to by the expressions $On(A, x)$, $On(x, Table)$ and z, where both x and z are quantified. What does this mean? Presumably *On* here is a function from blocks and tables, but what are its values? (Apparently, whatever they are, one gets a "state of the world" by applying the function $\circ$ to them, but that isn't much help. Thielscher casually distinguishes between 'situations' and 'states of the world' without explanation.)

Apparently, z here is supposed to be a *fact* (or, still more curious, "facts"), yet we have somehow managed to quantify over it (them?). How can one consistently quantify over facts, written using a logic, in that logic itself? (A rhetorical question, of course: one can't. Ref. Gödel).

Thielscher says that asserting $z \neq On(y, A)$ means that z does not have the "form" $On(y, A)$. But suppose we assert that (say) $On(B, A) = C$; now C does not have that form, yet it is equal to it. The author seems to confuse 'form' with 'reference' throughout the paper, in fact.

On page 2 we are told that $Holds(f, s)$ is a "macro". What does this mean?

The text ought to be rewritten and reorganised. First, the English is turgid and sometimes hard to follow. Second, the terminology being used should be carefully explained, since the uses here of "fluent" and "situation" (for example) are not those used by many other authors. Third, the logical semantics of the methods being used should be carefully explained; one cannot casually refer to a domain of quantification as consisting of "facts".

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